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Rāmāyaṇa
Between Archaeology and Text

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nahi śrutiśatamapi śīto ’gniprakāśo veti bruvatprāmāṇyamupaiti
— Śrīmat Śaṅkarācārya
Śrīmadbhagavadgītā Bhāṣya, 18.66

THIS note looks into *Rāmāyaṇa* from an archaeological perspective.[1] In order to do so I have had to necessarily deal with the textual history of the epic at some length. Lest someone is led to think that archaeology, being an empirical discipline, is infallible, I have also pointed out its limitations. In approaching such subjects, we are in a doubly disadvantageous situation: we have no securely dated uncontaminated "original" text, nor do we have foolproof archaeological evidence, and yet we seek to correlate them.

Epics and Criticism

In Sanskritic tradition, *Rāmāyaṇa* and *Mahābhārata* are called *itihāsa,* or "history".[2] The word *itihāsa* is also used in almost

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[1] References to the texts are from critical editions; those to Sanskrit plays refer to act, verse and dialogue. Thus, *Uttararāmacarita* 2.5.2 means 2nd dialogue after verse 5 of Act 2. *IAR* (followed by years) refers to *Indian Archaeology — A Review*, a publication of the Archaeological Survey of India.

[2] I am at a loss to see Thapar declaring that "[n]or is it [*sc. Rāmāyaṇa*] generally classified as an *itihāsa* — that which was believed to

all the New Indo-Aryan languages for English *history*. Though English *history* and Sanskrit *itihāsa* refer to different categories of knowledge, for an average Indian, *Rāmāyaṇa* and *Mahābhārata*

← have happened in the past" (1991: 142; also cf. Sankalia 1973b: 2; Arunkumar 1981: 162). At another place she makes a distinction between *kāvya* and *itihāsa* by pointing out that "[t]he *Rāmāyaṇa* is *always* [my emphasis] referred to as a *kāvya* unlike the *Mahābhārata* which is often called *itihāsa*" (Thapar 1978: 30, n. 9; also, cf. Sankalia 1973b: 19; Lal 1993: 7). I am at a loss because it is a quite common knowledge that *Rāmāyaṇa* was/is considered as *itihāsa*, cf. for instance, De and Hazra: "The present anthology consists of selections from what is known as Itihāsa and Purāṇa, the former comprising the two Great Epics of India, namely the *Mahābhārata* and the *Rāmāyaṇa* [. . .]" (1959: v). Khare writes the same: "संस्कृत-काव्यशास्त्र-परम्परा में [रामायण-महाभारत को] 'महाकाव्य' नहीं कहा जाता बल्कि वे 'इतिहास' काव्य की श्रेणी में रखे जाते हैं।" (1999: 13). Bhavabhūti explicitly calls *Rāmāyaṇa itihāsa* in his *Uttararāmacarita*, thus: *itihāsaṁ rāmāyaṇam* (2.5.2). Thapar appears to be underlining that if *itihāsa* is something "believed to have happened in the past" and *kāvya* an imaginary poem, then *Rāmāyaṇa* being a *kāvya* does not have any historical basis. We may remember here that the historian Kalhaṇa calls himself a *kavi* and his history a *kāvya* (1.3-5, 7; also Stein 1900: 22ff., 38ff.). Thus there is no binary opposition between the two. Second, though *Mahābhārata* is usually referred to as *itihāsa*, it is also called a *kāvya* (cf. *kṛtaṁ mayedaṁ bhagavankāvyaṁ paramapūjitam*| Ādiparvan, Appendix 1, l. 13; also ll. 34-35), apart from variously referred to as a Purāṇa (1.1.15), Upaniṣad (1.1.191) and even Veda (1.1.204-05; 1.57.74). Sanskrit aestheticians like Kuntaka while dealing with works based on "historical" themes cite examples ultimately coming from *Mahābhārata* and *Rāmāyaṇa* (cf. *Vakroktijīvita* 4.3-4 and 4.16-18). In his exegesis on the *kārikā*s (*ad* 4.16-17), Kuntaka in fact names these two works. Ānandavardhana also refers to these two epics when he talks about "historical" themes (cf. his auto-commentary on the *Dhvanyāloka* 3.11-14). For example, it is said that the vivid description of Aja's marriage in *Raghuvaṁśa* and the account of bringing the *pārijāta* tree (*Nyctanthes arbortristis* Linn.) from the heavens to please Satyabhāmā, as given in *Harivijaya*, are unknown to the "historical books", namely, *Rāmāyaṇa* and *Mahābhārata* (cf. *Dhvanyāloka*, ad loc.). Clearly, then, there was/is the Sanskritic convention to call both the "epics" *itihāsa*.

are histories in the English sense of the word, that is, "something that actually happened in the past". For them, Rāma, Sītā, Lakṣmaṇa, Kṛṣṇa once existed is beyond question; the invasion of Laṅkā by Rāma or the Mahābhārata war between the Kauravas and Pāṇḍavas were historical incidents (cf. Pollock 1993: 279). Since these two epics were/are thus purely historical for them, there had never arisen the need to examine them critically.

This should not be taken to mean that the ancient Indians did not have the faculty of criticism or reason.[3] A couple of examples should suffice. While denouncing Sāṅkhya philosophy in his commentary on *Brahmasūtra*, Śaṅkarācārya says we do know that Kapila (the founder of Sāṅkhya philosophy) is referred to in Śruti, that is in the Vedic literature (*sc. Śvetāśvatara Upaniṣad* 5.2), and *ipso facto* beyond challenge. Ācārya, however, argues that it is not possible to let Kapila's Sāṅkhya philosophy go unchallenged, for the tradition speaks of more than one Kapila, for example, the Sage Kapila who incinerated the sons of king Sagara. How can we be sure, asks Śaṅkarācārya, that Kapila mentioned in *Śvetāśvatara Upaniṣad* is none other than the founder of the Sāṅkhya school? (*ad.* 2.1.1).[4]

At the beginning of the fourth book of *Śrīmadbhagavadgītā*, Kṛṣṇa, while imparting the supreme secret knowledge to Arjuna, tells him that

> ere long I had given this knowledge to Vivasvān (Sūrya); he passed it on to his son Vaivasvata Manu; Manu transmitted the same to his son Ikṣvāku; and then it disappeared from

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[3] Bronkhorst in one of his assuming papers attributed what he called "rationality" in Indian tradition to the Greeks (Bronkhorst 2001). His "unusual" idea of rationality is clearly derived from Popper (1963: 26). Bronkhorst's fantastic proposition has been roundly, and justly, rejected (cf. Gerrow 2002; also cf. Turco 2005, 2011). For a discussion of Indian rationality, see Chakravarti (1999) and Ganeri (2001), to which Bronkhorst does not refer.

[4] However, Böhtlingk, Hopkins, and Olivelle accept this identification (cf. Olivelle's note ad loc. 1998: 625).

this world; but you being very dear to me, I will disclose it to you anon.

Arjuna in a critical spirit asks Kṛṣṇa,

aparaṁ bhavato janma paraṁ janma vivastataḥ |
kathametadavijānīyāṁ tvamādau proktavān iti ||

Vivasvān, the sun, was born much before you were born, so how is that you gave this knowledge to him?

Here Arjuna clearly exhibits a rational mindset and puts up a critical inquiry.[5]

Notwithstanding this, we must acknowledge that neither *Rāmāyaṇa* nor *Mahābhārata* were examined from such a critical perspective, ever.[6] It began after the Europeans had started studying these texts. They read, edited, printed, translated and studied these texts, usually with the help of the traditionally trained *paṇḍit*s (cf. Rocher 1986: 3 and n. 11, pp. 49-51; and now Dharamsey 2012: 23-30). Though their interest was not always purely scholarly (cf. Said 1978/2001),[7] they created an academic atmosphere in which such texts came to be studied and discussed

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[5] We may compare here Maitreya's question to Parāśara, "we have heard that Śrī came out of the Kṣīrasāgara during the churning of the ocean, then how do you say that she was the daughter of Bhṛgu and Khyāti?" (*kṣīrābdhau śrīḥ samutpannā śrūyate 'mṛtamanthane | bhṛgoḥ khyātyāṁ samutpannetyetadāha kathaṁ bhavān ||* — *Viṣṇu Purāṇa* 1.8.15)

[6] Unless we treat the "rectified" Jaina, etc. versions of *Rāmāyaṇa* as "criticism" of the original story (cf. Goldman 2005: 85ff.; for "rationalization" in the *Paümacariya*, see, Kulkarni 1980/2009: 233ff.).

[7] One of the unfortunate, and unwarranted at that, outcomes of this book is the increasing tendency on part of Indian (nationalist–Marxist) historians to read "conspiracy" in all colonial writings and to explain (away) all complex historical processes in modern India in the light of "colonial rule". The colonial rule and its policy of "divide and rule" have become, in this way, almost a metanarrative in recent historical writing.

in larger contexts;[8] in many cases, they helped to understand such texts in a better way. What follows here is largely in continuation of the critical work thus initiated on such texts, albeit from an archaeological perspective, with considerable help from the textual studies done by various scholars.

Archaeology: Scope and Pitfalls

Before we embark upon studying *Rāmāyaṇa* from an archaeological perspective, we need to understand what archaeology can and cannot do. Archaeology is comparatively a new discipline. It originated, evolved and matured in European countries in the nineteenth and twentieth centuries. It studies material remnants produced by human activities of the bygone ages. Humans are a species that produce the largest amount of waste on this globe, and some portion of this waste, generally solid and durable, gets buried in the soil, and given congenial conditions, survives for a long time. This garbage, when scientifically excavated and systematically retrieved, along with the monuments and other artefacts that have withstood the ravages of time, when properly studied, offer a hoard of new information about the past life-ways of the people who left them. The discovery of the Haṛappan civilization is an excellent example of the kind of miracles archaeology can perform.

While archaeology can and do perform miracles, it has its limitations too, which must not be overlooked. First, archaeologists cannot dig places where people presently live;[9] there is always a possibility that such places have valuable, even crucial, evidence buried beneath the ground. In India, because

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[8] D.D. Kosambi writes that "most of our source material was first collected, analysed, arranged by foreign scholars. To them we owe critical method, the first publication of authoritative texts, and archaeological exploration" (1948: 272).

[9] Lal and Dikshit, for instance, write, "[t]he ancient mound known as Garhi is largely under occupation by the villagers and it was with great difficulty that suitable spots were found for excavation" (1982: 26).

of the population pressure archaeological sites are destroyed and disappearing fast, thus the important evidence is getting lost forever. Second, digging reveals only a part, in fact a very insignificant part, of the original cultural material: only those things that are durable can sustain, and that too seldom in their original shape and quantity.[10] Third, when a site, usually a mound, is excavated, it is, as a rule, not excavated fully, for it is required that some area of the site be kept untouched for future archaeologists so that with superior techniques and advanced methodology they can retrieve data and interpret the evidence in better ways. Fourth, in countries like India, because of limited funds,[11] it is not always possible to carry out large-scale horizontal excavation of sites; most of the sites are excavated vertically and to ascertain the cultural sequence of a given site or a region. The retrieval of archaeological data in India is thus doubly restricted. Fifth, and this is important, there is no axiomatic correlation between human activities and archaeological remains (cf. Mate 1990; contra Paddayya 2008: 59ff.).[12] Next,

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[10] The utensils of the valuable NBPW were riveted and reused. This should explain why costlier metal objects are rare from earlier deposits. The limited number of metal artefacts does not necessarily mean lack of "advanced" technology or that the metal was rare: until very recently metal objects were melted to make new objects from it. The practice can be still seen in many parts of India today.

[11] Lal informs us that he did not excavate the sites "south of Chitrakuta, for obvious reasons of lack of time and money" (2002: 42).

[12] One example: literary evidence suggests the dispersal of the Vedic Aryans from the Saptasindhu towards the Gaṅgā basin in around the second half of the second millennium BCE. Lal writes, "[i]f this is true, one may reasonably expect the remains of their settlements in these areas" (1955: 6; cf. *per contra* Rau 1976: 50). Lal is here tacitly subscribing to this axiomatic correlation. One more example: since the exact meaning of *dhāna* being debated (cf. Wojtilla 2003: 44; in their recent translation of the text, Jamison and Brereton (2014: II.787) have not improved upon the previous attempts), it is safe to assume that *Ṛk Saṁhitā* refers to only one cereal, namely barley (*Hordeum vulgare*). Though the evidence of barley has come forth from many parts of India →

it is not always possible, despite the tall claims made by the practitioners of archaeology (usually in theoretical books with imaginary illustrations), to accurately interpret archaeological evidence.[13] Moreover, the interpretations are rarely free from biases; often, they are politically motivated. We should also bear in mind that before the mid-1970s, and indeed even after that, site formation processes were poorly understood, if at all. Site formation processes study how archaeological records come into existence and how they are modified by natural and human agencies after their discard and subsequent deposition (Schiffer 1972, 1976; for Indian context, see Petraglia 2002). Excavators often interpret archaeological data as if they were found *in situ* and did not undergo any change after they had been discarded. But this is not the case: in both sedentary and migratory societies the disposal of waste is, as a rule, "outside their use location" and in case of advanced sedentary societies the same is much more systematic and organized (Murray 1980). This means most of the artefacts are found "out of context". Inferences based on such data can, therefore, be positively delusive. It would be advisable if the reader keeps these limitations in mind while reading the following pages.

← (Kajale 1991), it is from the Haṛappan and from the Neolithic sites in Baluchistan that barley forms a lion's share. For instance, at Mehrgaṛh, period I, barley comprises more than 90 per cent of the total grain evidence (Constantini 1984).

[13] When I say that it is not always possible to *accurately* interpret an archaeological record, what I mean is it is not always possible to decipher its *original* meaning. In post-processual archaeology multiplicity of interpretation is praised. But that means your interpretation is as useful, or as *useless* if you wish, as mine. Though the author of an artefact is dead, and literally so, "the death of the author" (à la Barthes) — by which we can neatly separate, or we *think* we can neatly separate, the author and his work — does not hold good in archaeology. The intention of the author, in archaeology, however concealed, is crucial. I am discussing ramifications of this position in a separate note titled "Archaeological Interpretation(s): Between Barthes and Sainte-Beuve".

There are then some issues particular to archaeology as practised in India. First, when sites are excavated only important — that is, what the excavator deems important — results are announced, generally, in the *Indian Archaeology — A Review* and elsewhere; detailed reports of the excavations are not always published; some are published so late that they become less reliable. More often than not, the results of excavations consist of cultural sequences alone; other antiquities, which may throw welcome light on the material conditions of a given period, are seldom adequately studied.[14] Excavated material though systematically housed, is not easily accessible to researchers.

Despite limitations, archaeology as a discipline has immense potential to reconstruct history. No wonder that soon after the potential importance of archaeological findings was realized, attempts were made, by professional archaeologists and amateurs alike, to ascertain the historicity or otherwise of the mythical and traditional accounts. Heinrich Schliemann's digging at Hisarlık (which he identified as Troy) is a well-known, if controversial, example of such attempts (cf. Traill 1995). More recently, Hershel Shanks, editor of *Biblical Archaeology Review*, announced the sensational discovery of an ossuary with the Aramaic inscription *Yaakov bar Yoseph, Achui de Yeshua* (Eng. "James, son of Joseph, brother of Jesus"). As claimed by Shanks, this was "the first ever archaeological discovery to corroborate biblical references to Jesus". The discovery, it appeared, would put an end to the endless debates on the historicity of Jesus. As anybody's guess, the discovery was a hoax (cf. Silberman and Goren 2003). Such uncritical use of archaeology, however, should not dissuade one from correlating archaeology and tradition.

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[14] In an otherwise admirable report on Chalcolithic Nāvḍāṭolī, Sankalia's discussion on the possible connections between the central Indian cultures and the Iranian ones elicited this remark from the reviewer: "It is a pity that with so much solid evidence to work with, such space is devoted to unprofitable speculation" (Glover 1974: 481).

When we think of correlating archaeological evidence with literary data — or for that matter with oral traditions — or the other way round, a methodological question comes up, namely, whether one should proceed from archaeology to text or from text to archaeology? To put differently, should our approach be that of archaeology-based text or text-based archaeology? Usually it is on the basis of texts or legends that archaeological investigations are taken up. It must, therefore, be made clear that when we give more importance to archaeology over text, it is from the point of view of interpretation and dating alone.[15] In India, where texts in certain genres were constantly revised and rewritten, and the dating of most of the literary texts, right from the earliest text, *Ṛgveda,* is heatedly debated, Sankalia rightly remarked that "[o]ur ancient literature should not be used for dating monuments and sites, but it should be just the reverse. For clearly, we have much better evidence to date our monuments than to date our literature" (1982a: v).

These words of a great archaeologist, who was also a good Sanskritist, admirably explains what is "archaeology-based text" approach.

Traditions and Archaeology in India

In India, much before Schliemann's work in Anatolia, Cunningham had quite diligently followed the accounts of the Chinese travellers in order to locate the tracks trodden and places visited by them (Cunningham 1871/1924). John Marshal's obsession for the Greeks — as he confessed later, he was "filled with enthusiasm for anything Greek" (1951: xv) — led him to search for their material remains in the north-west India. For a long time, due no doubt to the phenomenal discovery of the Haṛappan Civilization, other archaeological activities in India came to a halt as it were. After Leonard Woolley's severe criticism of the Archaeological Survey of India

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[15] To me, the argument and thesis of Schopen (1991) are, though related, entirely different.

for its preoccupation with the Harappan Civilization, and after
the appointment of Mortimer Wheeler as Director General of
the Survey, archaeological activities in India were resumed in
a more systematic and scientific fashion. Many new vistas of
archaeological investigations opened up. One such opening,
though in no way inspired by Woolley or Wheeler, was what
came to be called "epic archaeology". It was B.B. Lal who first
thought of ascertaining the correctness or otherwise of ancient
Indian traditions with the help of archaeology. Lal systematically
explored places either associated with[16] or mentioned in
Mahābhārata, for example, Hastinapur (anc. Hāstinapura,
Uttar Pradesh (UP), Indrapat or Purānā Qilā (anc. Indraprastha,
Delhi), Tilpat (UP), Bairāṭ (anc. Virāṭanagarī, Jaipur, Rajasthan),
Barnāwā (anc. Vāraṇāvata, Meerut, UP), Kurukṣetra (Haryana),
Bāghpat (UP), Pānipat (Haryana), Ahichchhatra (UP), Kāmpil
(UP), Rājā Karaṇa-kā Qilā (Punjab), and Mathurā (UP). At all
these sites, the lowest levels of the deposits were distinguished
by the ubiquitous presence of a thin, fine, wheel-thrown, well-
fired grey pottery painted over in black colour, hence named
the Painted Grey Ware (PGW). The authors of the PGW culture
— as was the custom of the time to name a culture after the
distinctive pottery type[17] — were familiar, among other things,

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[16] Cf. "And even sites not mentioned in the texts but associated with the
story through local tradition were not spared. [. . .] The whole idea
of the investigation was not to leave out any site which came on the
way and was believed to be associated with the *Mahābhārata* story"
(1973: 2).

[17] The PGW was first reported from Ahichchhatra (org. Adhicchatra) in
1946 (cf. Ghosh and Panigrahi 1946: 38, 40-41, 58-59). The common
types of this ware are dish, bowl and *loṭā*. The PGW, if distinctive, is
not prominent compared to the pottery repertoire of the culture — it
forms at the most only 10 per cent of it. The other wares of this culture
are the black-slipped ware, black and red ware, and red ware, which
is very common (Lal 1989b: 107). Converse divided the pottery types
into the finely made, thin, at times slipped, red ware; thick grey ware (a
sub-variety of this ware is the black-slipped ware); medium heavy ware
                                                                          →

with iron — though not at the early stage perhaps (cf. J.P. Joshi 1978: 99-101; Lal 1980: 79; Makkhan Lal 1986: 93) — and horse (for a compact survey of the PGW cultures, see Lal 1992).

Lal later on excavated Hāstinapura,[18] the capital of the Kurus, and Purānā Qilā, traditionally identified with Indraprastha, the capital of the Pāṇḍavas. He also found certain archaeological evidence in support of the textual data. For instance, the Purāṇas inform us that during the reign of Nicakṣu,[19] when the waters of the River Gaṅgā inundated the capital, it was relocated to Kauśāmbī (*gaṅgayāpahṛte tasminnagare nāgasāhvaye tyaktvā nicakṣurnagaraṁ kauśāmbyāṁ sa nivatsyati*, etc. cf. Pargiter 1913: 5, 65 *ad finit*). Archaeological evidence of the flood[20] were found at Hāstinapura and so was attested the continuity of the PGW culture at Kauśāmbī, where a developed form of the PGW was noticed in the lower strata.

On the basis of his findings at Hāstinapura, Lal quite reasonably, if tentatively, equated the PGW people with the later Vedic people, for not only the time bracket of the PGW and

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← with red slip on the exterior and grey on the internal wall (including the reserve slip ware). The black-and-red ware, Converse informs us, could not be located at the site (Hāstinapura) or at Safdar Jung (1978: 480).

[18] Wrongly spelt, and consistently so, as Hastināpura. *Mahābhārata* calls it Hāstinapura, a name derived from its founder Hastin (cf. 1.90.36), who was the tenth king up the line from Śāntanu, the great grandfather of the Kauravas and Pāṇḍavas. The great grammarian Pāṇini also is familiar with the name Hāstinapura only (cf. 6.2.101). I have retained Hastināpura in quotations, etc. though.

[19] His genealogy is as follows: Arjuna → Abhimanyu → Parikṣīta → Janmejaya → Śatānīka → Aśvamedhadatta → Adhisīmakṛṣṇa → Nicakṣu.

[20] Lal's interpretation and claimed evidence of floods at Hāstinapura were not acceptable to scholars like Das (1969). The floods have otherwise been a constant menace in the Indian subcontinent, and have been repeatedly encountered in archaeological contexts (cf. Biswas 1994; for the period we are dealing with here, see, ibid.: 265ff.).

later Vedic texts but also the sites of the PGW and the places mentioned in the later Vedic texts matched only too well. The findings and the interpretations, if true, were indeed startling. Even so, the excavator ended his report with a caution, which is curiously absent in later discussion on Lal's pioneering work:

> *the evidence is entirely circumstantial and until and unless positive ethnographic and epigraphic proofs are obtained to substantiate the conclusions* [put forward above], *they cannot but be considered provisional.*
>
> — Lal 1955: 151; original emphasis

A. Ghosh in his "Notes on the Report" observed the compelling evidence on the suggested identification. He wrote:

> [T]he distribution of the pottery is virtually co-terminous with the land Brahmāvarta and Brahmarshi-deśa [. . .]. The date of the pottery is no less significant, for, if the "conventional date" of the entry of the Aryans into India, viz. 1500 B.C., is accepted [. . .], they would well have reached the Gangetic plain a few centuries later. — 1955: 2

Nevertheless, while summing up, he remarked, mirroring Lal's scepticism, that it would be "premature to hold that the latter [*sc.* the PGW] people were no other but the Aryans" (ibid.: 3). He cautiously wrote:

> The following report [*sc.* of Hāstinapura] often refers to the *Mahābhārata* and the place-names mentioned in it and brings into prominence the fact that the Painted Grey Ware is found at the sites associated with the story of that epic. [. . .] But a word of caution is necessary, lest the impression is left on the unwary reader that the Hastināpura excavation has yielded archaeological evidence about the truth of the story of the *Mahābhārata* and that here at last is the recognition by "official archaeology" of the truth embodied in Indian traditional literature. Such a conclusion would be unwarranted. Beyond the facts that Hastināpura, the reputed capital of the Kauravas, was found to be occupied by a people whose distinctive ceramics were the Painted Grey Ware in a period which might

roughly have synchronized with the date of the origin of the nucleus of the *Mahābhārata* story, that this occupation came to an end with a heavy flood and this Ware is found at many early sites, some of which are connected, either in literature or by tradition, with the epic-heroes, the excavation has no bearing on the authenticity or otherwise of the epic tale. It is indeed tempting to utilize archaeological evidence for substantiating tradition, but the pitfalls in the way should be guarded against, and caution is necessary that fancy does not fly ahead of facts.

— ibid.: 2-3

This indeed was a prudent note, for it reminds us that unless unambiguous evidence is found, we must not jump to conclusion from circumstantial evidence.[21] A similar warning was issued by Sankalia. Commenting upon the findings of the excavation at Dvārakā, he wrote that though the evidence of the sea encroaching upon Dvārakā was beyond doubt, the same did not support in any way the story of Kṛṣṇa's migration to Dvārakā or Dvārakā being his capital (1973a: 15; also cf. 1966: 17).

Compare now this remark of Wheeler with the scepticism of Ghosh and Sankalia: In his report of Chārsada excavations,

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[21] Later on, in the light of later researches, Ghosh changed his view and accepted the proposed identification to be secure. He wrote: "it would suffice to emphasize that the geographical horizon of the later Aryans is conterminous with that of the ware; there is also a remarkable chronological proximity between the dates of the beginning of the ware and the later Vedic age, which no critical scholar would place before the start of the first millennium BCE There can, therefore, be no reasonable doubt in ascribing the Ware to the later Aryans, together with the vast mass of red ware associated with it" (1973: 6). Parpola puts the Middle Vedic Period around 1000 BCE and correlates it with the PGW and the Kuru state (1984: 453ff.). Witzel follows Parpola without referring to him (Witzel 2003: 35ff.). It is widely accepted that it was Lal who proposed the connection between the PGW and the later Vedic people (cf. Brockington 1998: 159-60). R.S. Sharma thinks differently, however. In his opinion, it was he who suggested this identification and that it was "subsequently" "favoured" by A. Ghosh and B.B. Lal (cf. Sharma 1996: 43-44 and n. 7 on p. 50).

Wheeler, after delineating the early stages of habitation at the site, wrote that "the early history [of Chārsada] thus runs parallel to that of Taxila (Bhiṛ Mound) in the Punjab, and the assertion of the *Rāmāyaṇa* that Taxila and Pushkalāvatī were founded at the same time accords with the archaeological evidence" (Wheeler 1962: xii, 15). Such bold approbation from Wheeler was welcome in that the tradition survived in *Rāmāyaṇa*, and by implication in other sources, may well be historical.[22]

The archaeological data recovered from the excavation at Hāstinapura and the discovery of identical material culture from the lowest levels at other sites associated with *Mahābhārata*, either orally or textually, convinced Lal that "the *Mahābhārata* may not altogether be a concoction" (1975: 312; cf. 2002: 39-40, 52). On similar lines, therefore, the project of the archaeology of *Rāmāyaṇa* was conceived. However, the problems of doing archaeology of — or extracting historical information from — unwieldy texts such as *Rāmāyaṇa* and *Mahābhārata* are very many and in some sense insurmountable.

Textual Problems of the Epic

The various parts of the epics, *Rāmāyaṇa* and *Mahābhārata,* were composed at different intervals, not quite unlike the *Iliad* and *Odyssey* ascribed to the imaginary author "Homer".[23] The

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[22] The account of founding of these twin cities refers not to the earliest cities but to the ones founded by the Indo-Greeks in the second-first century BCE in the so-called "chessboard pattern" (cf. Wheeler 1962: 13-14). In a later publication Dani informs that this account of *Rāmāyaṇa* "has been disproved by new archaeological evidence" (1986: 40).

[23] Cf. Finley 1966: 5ff. Even the name "Homer" is anything but Greek (for conjectural etymologies of the name "Homer", see, Ford 2002: 113, n. 4). We do not hear of this "Homer" before the sixth century BCE. Herodotus (*c.* 440 BCE) is the first person to refer to these bardic collections as *Iliad* (Ford 2002: 148). Plato's quotations from the epics are often not found in the present-day texts of the *Iliad* and *Odyssey*, showing the texts were still in a flux in as late as 350 BCE (cf. Bouvier 2003: 60; also cf. 2004).

composers of these different parts of the epics depicted, naturally, the conditions and things as known to them, both in terms of time and space. For example, the reference to the inscribed finger ring of Rāma suggests that the author of that part of the epic was familiar with signet rings that were first introduced in India by the Indo-Greeks.[24] Sitā's vow of offering a hundred pots of wine to the River Yamunā shows poet's acquaintance with Roman wine, definitely a luxury import in India, which came in Roman amphorae during the palmy days of the so-called Indo-Roman trade, that is, in the early centuries of the Common Era (cf. Sankalia 1982b).[25]

One must first, therefore, undertake to discern the layers of the texts and to date them before or by correlating them with archaeological data, which is simply a daunting task. Even if one succeeds in untangling the hopelessly mixed up strata of the texts,[26] the problem is far from solved, for the texts, or parts of the same, do inescapably reflect the *Weltanschauungen* and concerns of the interest groups who composed them. Such texts

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[24] Interestingly, a round signet bearing legend in Aśokan Brāhmī was found from Ayodhyā (cf. Dikshit 2003: 116).

[25] The sherds of the Rouletted Ware (*c*. 100-200 CE) were recovered from Ayodhyā. This, according to the excavator B.B. Lal, was "perhaps the most inland context" in which the ware was found in northern India, and which suggests some sort of contact with the Western world, and presumably also of flourishing trade (cf. *IAR 1976-77*: 53; 2002: 44).

[26] Sukthankar writes: "[T]here are numerous passages, short and long, that are found in one recension and are lacking in the other [. . .]. No convincing proof can *in general* be given to establish either the originality or the spuriousness of any given passage of this type. What may fairly be regarded as interpolations are in general so ingeniously fashioned and so cunningly fitted in that, except under very favourable circumstances, the intrinsic (contextual) evidence is inconclusive" (1933/1944: 98-99, original emphasis; also cf. p. 126). The same applies to *Rāmāyaṇa*, too. For necessarily-not-very-successful attempts with regards to *Rāmāyaṇa*, see Guruge 1960: 31-35, 38-41; Brockington 1984: chap. 2, pp. 16-61, and tabulated summary on p. 330.

serve the political–religious–social ends of such groups, usually the brāhmaṇas or even a sub-segment of them, and may not depict a correct picture of the society.[27] The episode of Śambūka, for instance, was concocted especially to deny the śūdras the right of penance, though the selfsame text also portrays the mixed-caste Śrāvaṇa performing Vedic rituals (2.58.28). He is the son of a śūdra mother by a vaiśya father (2.57.37), who according to Manu would be called a Karaṇa (*Manusmṛti* 10.6; Kullūka denies them the caste of their fathers, ad loc.; also cf. *Amarakośa* 2.10.2), and, as a rule, not allowed to perform *sandhyā*. The long interpolated passages of wailing by Tārā and Mandodarī after the deaths respectively of Vālin and Rāvaṇa, as well as the fire ordeal of Sītā, were forged specially to rivet the idea of a chaste woman (*satī*) (cf. Sen 1952; Bhatt 1956).

Lastly, the description of material culture in the texts, given the unbridled poetic fancy of the poet, may well be a far cry from the actual conditions. The celestial craft Puṣpaka is an excellent example. The same may apply to socio-economic conditions, too. Dev Raj Chanana's study of *Rāmāyaṇa* shows that agriculture was not practised in or around Laṅkā, nor were animals domesticated (1963: 8-9, 11-13, 24). This *rākṣasa* culture, as described by the poet, scarcely seems to be more advanced than that of the savages ("savagery" in archaeo-anthropological sense of the term, that is hunting-gathering economy), and therefore without any, or at the best minimum, surplus. Such economy is simply incapable of producing towering palaces, beautiful gardens,

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[27] Sukthankar's masterly treatment showed how the Bhṛgus, as the transmitters and composers of *Mahābhārata*, had inflated the epic beyond imagination (1937/1944; cf. also Shende 1943a, who suggested that along with the Bhṛgus, the Aṅgirasas were equally responsible for the additions). In the case of *Rāmāyaṇa* also, the role of the Bhṛgus is indicated (cf. Shende 1943b; also cf. Shastri 1978). Goldman, after carefully examining the Critical Edition of *Rāmāyaṇa*, came to the conclusion that Bhṛguization can be seen only in Bālakāṇḍa and Uttarakāṇḍa. Curiously, Goldman does not make any reference to Shende's article (cf. Goldman 1976).

gigantic forts, etc. that are so vividly described in the text. This means the picture of Laṅkā is anything but realistic. References to the natural wealth of Laṅkā made in *Rāmāyaṇa*,[28] which has become legendary in Indian psyche, in terms of pearls, precious stones, valuable metals, and above all gold, are surely later interpolations, which in all probability were inserted after its unfortunate identification with Ceylon.[29]

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[28] Thapar fancies that it may have been "a vague folk memory of the rich cities of the Bronze Age past"! (1978: 20).

[29] According to Goldman, "the poet knew of an island kingdom, [. . .] said to lie some distance off the coast of the Indian mainland" but given his limited geographical knowledge, he placed Laṅkā in around the Vindhya Range. According to him, it is "unlikely" that "Laṅkā was conceived of as lying within the boundaries of peninsular India" (Goldman 1984: 28). Goldman seems to be equating Laṅkā with Sri Lanka (so does Guruge 1960: 68-69). However, as argued by Sankalia, the toponym Laṅkā for the island is not attested in inscriptions earlier than the sixth century CE (1977; cf. Gokhale 2004: 139; Bapat also showed that the early Pāli sources do not know Ceylon as Laṅkā; the names are uniformly either Tamba-paṇṇi-dīpa or Sīhala-dīpa. *Ap*. Shah 1976: 109). The Ceylonese chronicles *Dipavaṁsa* and *Mahāvaṁsa*(*c*. 400 CE) do refer to their country as Laṅkā, but it is not attested anywhere in Indian records before the sixth century CE. The Bodh-Gayā inscription of Mahānāma that equates Laṅkā with Ceylon is fairly late, that is of 588-89 CE (cf. Sankalia 1977: 209; Sircar 1980/2009: 333); moreover, it was inscribed by a native of Sri Lanka who called their island by that name. Laṅkā of *Rāmāyaṇa* is doubtless earlier than this date and therefore it cannot be identical with Sri Lanka. The suggestions of Wüst on the etymology of Laṅkā (1965) are also helpful in this connection, in that they can more appropriately be applied to the geographical description of Laṅkā as given in *Rāmāyaṇa* (cf. Mankad 1965: LXII) rather than to Sri Lanka. Sircar's copious — and irrelevant — references are unsuccessful to prove the identity of Ceylon and Laṅkā (1980/2009: 327-33). For a survey and argument that Laṅkā is not Ceylon, see Shah (1975: 31-50; 1976). The Old Tamil text, *Akanāṉūṟu*, is said to refer to Kōṭi (*sc*. Dhanuṣkōṭi) as "the place from which the victorious Rāma crossed over to Laṅkā" (cf. Hart 1975: 61f.; Parpola 2002: 362). The poem is dated to the second-third century CE. If the dating is correct, this should be →

Composed and revised over a millennium, the textual evidence is thus not reliable in its entirety. As M.C. Joshi has laconically put it, the text lacks "(a) the objectivity, and (b) the contemporaneity" (1978: 98). Not that Lal was not aware of these problems, for he clearly wrote that the correlation between archaeology and the epics

> would have been easier had the two epics [. . .] been contemporary with the events narrated in them. Unfortunately, however, these are very far removed from the probable time of Krishna and Rama. The texts, as available to us now, are broadly assignable to circa 3rd-4th century AD. Besides, they evince a full play of poetic imagination. All these make the path of the historian extremely difficult.   — 1975: 311

Scholars consider a large portion of Bālakāṇḍa[30] and Uttarakāṇḍa as a whole to be later additions.[31] In all probability, the *Rāmakathā* so-called, consisted of Ayodhyā, Araṇya, Sundara, Kiṣkindhā and Yuddhakāṇḍa. This is not to suggest that there are no interpolations in them.[32] The critical edition of *Rāmāyaṇa*,

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← the earliest reference to Rāma's association with Ceylon. However, it is possible that since the local population called their country Laṅkā, which was also the name of the Rāvaṇa's place, the neighbouring Tamils, familiar with the epic, invented the story of Rāma's invasion of Ceylon.

[30] Some portion of Bālakāṇḍa formed part of the original story (cf. Jacobi 1893: 55ff.; Goldman 1984: 61ff.).

[31] The Brockingtons translated the five *kāṇḍas*, namely Ayodhyā, The Forest, Kiṣkindhā, Beauty and War, which in their opinion consisted of "an early form of the *Rāmāyaṇa*" (cf. Brockington and Brockington 2006).

[32] Naïve dependence on these *kāṇḍas* for assertion or denial of a fact, therefore, becomes problematic. For instance, Lal points out that Kṛṣṇa is mentioned at the event of Sītā's fire ordeal in Yuddhakāṇḍa (6.105.14; 6.105.25), which is "never regarded as a later addition like the Bala-kanda and Uttara-kanda", and hence Rāma may well have been later than Kṛṣṇa (Lal 1988: 62; 2002: 50). This is a desperate attempt on Lal's part to interpret textual tradition to conform to archaeological findings. But, as pointed out earlier, the episode of

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though a commendable achievement in itself, is not very helpful
in reconstructing the "original" or "Ur"-*Rāmāyaṇa*. The critical
edition of any text aims not at the Ur-text but tries to get as
near as possible to the oldest recension that can be retraced on
the basis of the available manuscripts;[33] the redactor is bound
by the manuscript tradition and cannot be go beyond it, even
when he is fully aware of the interpolations in the critically
constructed text.[34] For example, the nonsense episode of feast
and merrymaking by the army of Bharata — and that too in the

← Sītā's fire ordeal is an interpolation. The same applies to Joshi's
objection to Sankalia's equation of Rāvaṇa with a Gond chief: "[a]
Gond, traditionally, could be called a Śabara and not a Rākṣasa.
And Rāvaṇa was not an ordinary Rākṣasa but a Brahma-Rākṣasa
well-familiar with the Vedic lore" (M.C. Joshi 1978: 101). Rāvaṇa's
reputation as a Brahma-Rākṣasa,well-versed in the Vedas, is based on
Uttarakāṇḍa, which, as a whole, is a much later addition, unknown
to the original story, and this makes Joshi's objection ineffectual (cf.
also Goldman and Masson 1969).

[33] I cannot do better than quote the great Sukthankar. He wrote: The
critical edition presents "*a version of the epic as old as the extant
manuscript material will permit us to reach* with some semblance of
confidence. It is, in all probability, not the best text of the Great Epic,
possible or existing, nor necessarily even a good one. It only claims to
be *the most ancient one according to the direct line of transmission*, purer
than others [. . .]" (1933/1944: 129; original emphasis; also cf. p. 97).

[34] Thapar states, "even the Critical Editions of both [*sc.* the *Rāmāyaṇa*
and the *Mahābhārata*] [. . .] *have not been able to prune the texts* to
the original or approximate epics" (1984: 15; my emphasis). This is a
common misunderstanding about critical editions. Such "pruning", in
the words of Sukthankar, was "beyond the scope of this edition, since
the entire manuscript evidence unanimously supports the conflation,
which is too old and deep-rooted to be treated by ordinary principles
of textual criticism" (1933/1944: 110). Sankalia is equally innocent.
He writes: "The episodes of Rāma's *pādukā* and finger-ring are known
[…]. Everyone, even scholars, have accepted these as historical facts, so
much so that even the Editors of the Critical Editions of the *Rāmāyaṇa*,
[. . .] have included these episodes and even others, in the respective
editions [. . .]" (1982b: 337).

presence of Bharata and the three widowed wives of Daśaratha —
on their way to meet Rāma, at the hermitage of Bharadvāja after
the recent death of Daśaratha and the banishment of Rāma, is an
apparent interpolation (cf. Sankalia 1982b), yet it is retained in
the critical text (cf. 2.85) only because the manuscript evidence
uniformly supports it.

The oldest available manuscript of *Rāmāyaṇa*, written
in Newārī script, dated to 1020 CE,[35] belongs to the Northern
Recension (or, more correctly, to the North-western Recension,
cf. Shah 1980: 94); this NW Recension, according to the majority
of scholars, underwent many changes. In the Southern Recension,
which is believed to have preserved the original text better, the
manuscripts are of a much later date; the earliest one bears the
date of 1416 CE.[36] D.R. Mankad, the editor of Kiṣkindhākāṇḍa, in
his Introduction points out that even before the Ur-text bifurcated
into the Northern and Southern Recensions there had been
many interpolations and excisions (1965: XLI). After carefully
comparing the episode of the Mandeha *rākṣasa*s with the ones
given in *Brahmāṇḍa* and *Vāyu Purāṇa*, Mankad concludes that
"several original readings [of *Rāmāyaṇa*] are not traceable in any
of the Mss collated by us. Thus the R[*āmāyaṇa*]-text tradition
goes far beyond these Mss" (Ibid.: XLIII). He rightly deduced that
"[w]e are, therefore, very far from the original R[*āmāyaṇa*]-text"
(ibid: XLI).

Discounting the interpolations, *Rāmāyaṇa* is a fairly early
and homogeneous text. Modern scholarship accepts and supports
the perceptive observations made by Jacobi more than a century

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[35] The date is mentioned at the end of Kiṣkindhākāṇḍa. Shah writes,
"[w]ho copied the remaining kāṇḍas is not known, nor do we know
when they were copied" (1980/2009: 94).

[36] Before the discovery of this manuscript (called M_9), the earliest one
was dated to 1512 CE comprising Bālakāṇḍa and Ayodhyākāṇḍa. M_8,
which contains Uttarakāṇḍa alone, is said to be as old as early eleventh
century (cf. Shah 1980/2009: 95-96).

ago on the dating of *Rāmāyaṇa*,[37] and puts the oldest parts of the epic between 750–550 BCE (cf. Goldman 1984: 22-23). While it is comparatively easy to determine and date the kernel of the Rāma story, it is quite difficult to visualize the kind of material culture that could have been associated with the original story. And therefore, needless to say, the project to correlate the same with the archaeological evidence found from such sites as are mentioned in *Rāmāyaṇa* becomes precarious.

Archaeology of the Rāmāyaṇa Sites

In 1975, B.B. Lal took up the project "Archaeology of Ramayana Sites" to excavate the places mentioned in *Rāmāyaṇa* to ascertain the historicity or otherwise of the legend of Rāma. Under the Project, the site of Ashrafi Bhavan, in Ayodhyā, was excavated first. Subsequently, other sites like Sringraur (anc. Śṛṅgaverapura), where Rāma et al. passed their second night after their banishment from Ayodhyā; Bharadvāj Āśram (Allahabad, UP), where they spent their fourth night; Pariār (? < Parihāra, meaning "abandonment", referring to the stranding of Sītā by Lakṣmaṇa, in Unnāo, UP); Biṭhūr (the place where Vālmīki reputedly had his hermitage and where Sītā stayed), were also excavated.[38]

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[37] For instance, and I summarize from Goldman (1984: 20ff.), *Rāmāyaṇa* speaks of Viśālā and Mithilā as two different settlements, with two different rulers, whereas by the Buddha's time they had conjoined to form the one city-state of Vaiśāli, and we hear nothing of Mithilā; it does not make any reference to the great city of Pāṭaliputra (estd. *c.* 490 BCE) while narrating the origins of Kauśāmbī, Mahodaya, Dharmāraṇya, Girivraja and Vasumatī (1.31.3-8), where its mention was all but unexpected (cf. Goldman 1984: 21 n. 22); the epic mentions Ayodhyā as the capital of Kosala, but Prasenajita, the king of Kosala and a contemporary of the Buddha, rules not from Ayodhyā but from Śrāvastī; the name Sāketa, so prolifically referred to in the Buddhist literature, is nowhere to be met with in the epic (also cf. Sankalia 1982a: 7-9). This, of course, does not mean that the present text of *Rāmāyaṇa* is equally old.

[38] Though the last two sites are not mentioned in some publications, for
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AYODHYĀ[39]

Lal's was not the first attempt to do the archaeology of the *Rāmāyaṇa* sites; a few attempts were made before: B.B. Lal himself had explored Ayodhyā (and some other sites associated with the epic), in 1955-56, soon after his excavation of Hāstinapura.[40] The survey brought to light sherds of the Northern Black Polished Ware (NBPW),[41] fine grey ware without paintings and "large-sized bricks with typical finger marks" (*IAR 1955-56*: 71). As early as 1964, Sankalia had suggested excavating "principal places associated with Rama — such as Ayodhya" for a better understanding of the epic (1964/1977: 278). After this, a very cursory exploration was carried out by Vijai Shankar Dubey (cf. *IAR 1961-62*: 53).

A.K. Narain and his team were the first to excavate the site of Ayodhyā, in 1969-70, with the view of ascertaining its cultural sequence. In all, three digs were made at Jain Ghāṭ, Lakṣmaṇ Ṭekrī and Nal Ṭīlā, and were referred to in the report respectively as Ayodhyā-1, Ayodhyā-2 and Ayodhyā-3. At Ayodhyā-1 and 2,

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← example, Lal 1987: 79, they were referred to in an earlier publication of Lal, namely, राम-कथानक संबंधी स्थानों का पुरातात्त्विक अन्वेषण (Gwalior, 1975), which I have not seen (I quote this from M.C. Joshi 1978: 101, n. 1).

[39] Based on *IAR 1976-77*: 52-53; *IAR 1979-80*: 76-77.

[40] Any attempt to correlate archaeology and the epic must take into account the problems of identification of places mentioned in the text and their possible modern equivalents; else one would end up digging an altogether different site. The key site for archaeology of *Rāmāyaṇa* has to be Ayodhyā, naturally. Though a couple of scholars (e.g. M.C. Joshi 1978, 1982; Bakker 1986a, 1986b) have raised their doubts about the association of Ayodhyā of *Rāmāyaṇa* with the present town of Ayodhyā (Faizabad, UP), majority of the scholars, quite correctly, are convinced of the identification (cf. Hemant Dave, forthcoming).

[41] Though called Black, which by the way is quite common and found in abundance from all the NBPW sites, the polish differs and we do come across other colours like "steel-blue, silvery, golden, pinkish, gold-blue and brown-black" (Nigam 1961: 37).

three cultural periods were noticed. While the first two were continuous, there was a hiatus between the first two and the third. Interestingly, at Ayodhyā-3, "at a comparatively lower level than the remaining two cuttings [*sc*. Ayodhyā-1 and 2], the deposits of only the earliest cultural period were encountered". The earliest cultural period of Ayodhyā-3 could be taken to mean the earlier habitation of Ayodhyā, whereas the multiple cultural deposits of Ayodhya-1 and 2 represent the later Buddhist habitation of Sāketa. The remains recovered from the excavation were the NBPW, terracotta discs, balls, wheels, bone artefacts, and objects made of copper and iron. From the upper levels, human and animal terracotta figurines and a couple of Ayodhyā coins[42] were found (*IAR 1969-70*: 40-41). The excavation thus stratigraphically established what was known from the explorations at the site, that the earliest culture in Ayodhyā was the NBPW.

Under Lal's project, excavations were first carried out at Ayodhyā at no less than fourteen different places like the Ram Janmabhumi mound, the open area west of Hanumān Gaṛhī and near Sītā kī Rasoi from 1975-76 onwards. While the digs confirmed that the earliest habitation at the site was marked by the presence of the NBPW, it also revealed that the NBPW here had two distinct phases: the earlier, datable to the beginning of the seventh century BCE, was marked by some peculiar shapes that were not encountered in the second phase. The NBPW was discovered in association with the red ware and grey ware. The shapes of the red ware changed with time and so did the terracotta images with

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[42] The coins found from around the region of Ayodhyā are named after the region. Chronologically, they come after the city coins, which are extremely rare and bear legends in Prakrit (cf. Lahiri 1970: 59). The reports of coins bearing the Prakrit legend *ajudhe* suggest that the regional coins of Ayodhyā were preceded by the city coins of Ayodhyā (cf. *IAR 1967-68*: 65 and *IAR 1970-71*: 63). One more coin engraved with the legend *ajudhe* was reported from the Allahabad Museum but the reading was questioned (cf. Tripathi 1965 and Ahmad 1971).

greater variety and proliferation. The grey ware was interpreted as the "very late and degenerate phase" of the famous PGW.

As for the material culture, the first phase was represented by houses made of wattle and daub and mud; the second was by baked bricks, town planning, terracotta ring-wells (called *maṇḍalin* in Sanskrit, cf. Agrawala 1959: 30), a massive wall (interpreted as a fortification wall), and a deep ditch. This phase ended around the third century BCE but the site was occupied during the following periods. Though material belonging to the Gupta period was not discovered in earlier seasons at the site, a house as well as pottery ascribable to the Gupta period was unearthed in the later seasons.

A rich hoard of antiquities was excavated from the site. An important find was a grey terracotta figurine with the head shaven, elongated earlobes, and standing probably in *kāyotsarga* pose (the image is broken; only the upper torso has survived), and therefore identified as a Jina image (now on this image, see Lal and Srivastava 1981). This could be possible as Ayodhyā, being the birthplace of the first *tīrthaṅkara* Ṛṣabhadeva, was also a great centre of Jainism.[43] Among other important finds were the terracotta sealing of King Vāsudeva who was already known from his Ayodhyā series of coins,[44] and a coin of King Mūladeva of the same period, second century BCE.[45] A good number of

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[43] The image comes from the "levels ascribable to *circa* fourth century B.C. and is perhaps the earliest Jaina [*sic*!] figure of this kind so far found in India" (*IAR 1976-77*: 53). Elsewhere it is dated to "the beginning of the third century B.C., if not somewhat earlier" (Lal and Srivastava 1981: 329). The identification is not universally accepted, however (cf. Cort 2010: 38-39).

[44] Elsewhere Lal refers to the terracotta sealings of King Vāyudeva (*sic!* Vāsudeva?) and Vijayamitra (1989a: 32).

[45] We do not know if these kings Vāsudeva and Mūladeva were related to King Dhanadeva of the famous Ayodhyā inscription (cf. Sahni 1929). It is important to note here that a copper coin of Dhanadeva and a terracotta sealing bearing the legend (*dha*)*nadeva* were recovered from Śṛṅgaverapura (Lal 1993: 46).

cult images, comparable to those from sites like Ahichchhatra, Kauśāmbī, Piprahwā, Vaiśālī, were also brought to light. The discovery of a few sherds of the rouletted ware, according to Lal, suggests "large-scale trade and commerce at Ayodhyā in the early centuries of the [Common] era". To this we may add cylindrical pieces (weights?) of various sizes made of siliceous and semi-precious stones recovered from all levels. Pendants of crystal and valuable stones styled after animals and birds were among the other notable antiquities.

Ayodhyā was certainly a great centre during the third century BCE as the discovery of "an inverted lotus capital of Mauryan craftsmanship and bearing typical Mauryan polish, installed as *argha* supporting a Śiva-liṅga of a later period, in the Nagesvarnath temple" conclusively proves (cf. *IAR 1977-78*: 83). This capital, in all probability, belonged to one of the pillars erected by Aśoka.

The Gupta period at the site was followed by a gap; the site was "again occupied around the eleventh century".

After the demolition of the Babari Mosque, excavations were carried out at the disputed site by Hari Manjhi and B.R. Mani in 2003-04. They revealed no earlier deposits than those previously noticed.[46] Notwithstanding this, the date of the NBPW was pushed back to "the beginning of c. 1000 BCE" (Dikshit 2003: 116).[47] According to the excavators, the earliest date of the site is "c. 1300 BCE (1250 ± 130 BCE)" (ibid.: 117). During this period, the

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[46] Source: Dikshit (2003), who had access to Hari Manjhi and B.R. Mani's report "Ayodhya: 2002-2003. Excavation at the 'Disputed Site', Archaeological Survey of India, New Delhi (photostat copy)", which I have not seen.

[47] Dikshit writes: "As revealed in earlier excavations the Northern Black Polished Ware was again noticed in the lowest levels along with [the] grey ware, black slipped ware and associated red ware but instead of placing it [*sc.* the NBPW] between c. 600–300 B.C., the present excavators on the basis of material evidence and Radiocarbon dates placed the beginning to c. 1000 B.C." (2003: 116).

evidence of burnt clay with reed impression was found. Among "other minor objects" were reported "broken weights, ear-studs, discs, [a] broken animal figurine, [an] iron knife, glass beads, bone points and a round signet with legend in Aśokan Brāhmī" (ibid.: 116). The material evidence, as described by Dikshit, does in no way warrant an earlier date; as a matter of fact, they suggest a late date. Yet, Dikshit maintained that

> [the] pre-NBPW deposit has strengthened the Hindu myths and belief that the story of Rāma and Ayodhyā is earlier than the story of Krishna and Mahābhārata and Hastinapur. The earlier analysis of correlating them with [the] early Northern Black Polished Ware now stand [*sic*!] altered and goes more in favour of placing the Ramayana episode to [the] pre-NBPW horizon.   — ibid.: 117

NANDIGRĀM

Excavations at and around Nandigrām, some 16 km south of Ayodhyā, on the River Tamasā, the famed place from where Bharata ruled the kingdom as the regent of Rāma (cf. *Rāmāyaṇa*, 1.1.31c, 1.3.10b; 2.107.12c, 13d; 2.107.19c, etc.), also "revealed a co-eval antiquity, by and large, with Ayodhyā". It may be stressed here that the aforementioned evidence came not from Nandigrām, which is on the northern bank of the Tamasā, but from the mound called Rahet, on the southern bank of the river (*IAR 1976-77*: 53).

ŚṚṄGAVERAPURA[48]

Sringraur (ancient Śṛṅgaverapura), also known as Sūrya Bhitā, situated 35 km upstream of Allahabad on the Gaṅgā, is mentioned several times in *Rāmāyaṇa* (cf. 1.1.25c; 2.44.1d, 2.105.22c, 23a; 6.111.28c, 6.113.4a, 6.113.20a). This site was subjected to excavation from 1977 to 1986.

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[48] Based on *IAR 1977-78*: 54, 56; *IAR 1978-79*: 57-59; *IAR 1979-80*: 74; *IAR 1980-81*: 67-68; *IAR 1981-82*: 66-67; *IAR 1982-83*: 91-92; *IAR 1983-84*: 84-85; and Lal 1993.

Unlike other sites associated with *Rāmāyaṇa*, with the exception of Pariār, cultural deposits at this site went back to the black-slipped ware and Ochre Coloured Pottery (OCP), datable respectively to the eighth and eleventh century BCE (cf. Agrawal et al. 1981); the other wares being the thick red ware (a few sherds), black-and-red ware and burnished grey ware.

The OCP,[49] so-called, is represented here by the red-slipped ware of which two categories were noticed: one fine made (occasionally decorated with incised or appliqué designs or painting in black), and the other not well made and coarse. The shapes include jars, bowls, shallow basins, vases and dishes. Though baked and unbaked bricks are reported from the OCP layers from sites like Lāl Qilā (*IAR 1969-70*: 38 and *IAR 1971-72*: 46), none was discovered here; instead, remains of wattle and daub structures were noticed. In this period, the site was only partly and intermittently occupied. After a gap, in the next period, the black-slipped, black and red, and burnished grey ware appear, but the house structures remain the same. It was in the middle strata of this period that the sherds of the PGW were discovered. Beads of terracotta and jasper, bone-barbed arrowhead, pendants and points were also recovered from this period.

Though in the lower strata the PGW was not encountered, shapes reminiscent of the potteries associated with the PGW (at Hāstinapura) were detected in the OCP; in the lowest levels of the NBPW phase, a few sherds of the PGW were spotted along

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49 Also called the Ochre Colour Ware (OCW). The ware was found in association with copper objects at Lāl Qilā, (cf. *IAR 1969-70*: 38). Its association with the Copper Hoard Culture was confirmed at Saipai where it was revealed that the ware in effect was red slipped ware (cf. *IAR 1970-71*: 38). Earlier from Lāl Qilā "[a] large number of painted sherds showing designs in black over a red-slipped surface" were reported, which, as was subsequently proved, were nothing but the OCP (*IAR 1968-69*: 37; also cf. *IAR 1969-70*: 39). It has been argued that because of water-logging for a considerable period the colour of the pottery changed from red to ochre (cf. Lal 1968).

with the fine grey ware, with an NBPW-like treatment on the outer surface. This was interpreted as an "intimate relationship" between the PGW and NBPW. The NBPW period could be divided into three sub-phases at the site. The first sub-phase of the NBPW was devoid of baked bricks. In the second and the third sub-phases, the site expanded and with it were introduced coinage, weights, baked bricks and script.

Of special importance was the chance discovery of a massive tank built with baked bricks datable to the second century BCE. It was in use till the beginning of the Common Era. A structure with multiple rooms with successive brick floorings, with a drain and a soakage pit longer than 4 m, was also unearthed.

In the subsequent period, cultural deposits belonging to the Kuṣāṇa and Gupta times were detected. The excavators also observed "widespread disturbance" at the site during the Gupta period. The fourth period is called the post-Gupta period and marked by the cultural material datable to the sixth to thirteenth centuries.

Apart from a good number of images of the Goddess Hāriti and miniature terracotta lamps, the site also yielded beads of various shapes and material, terracotta objects, ring-wells, copper utensils, uninscribed cast coins, silver and copper punch-marked coins, Ayodhyā coins, Kuṣāṇa coins and Gāhaḍavāla silver coins. Some beautiful terracotta images, seals and sealings, etc. were also found.

PARIĀR[50]

Located on the left bank of the Gaṅgā, Pariār (Dist. Unnao) is traditionally considered to be the place where Lakṣmaṇa abandoned Sītā; on the opposite bank is Biṭhūr, where the *āśrama* of Vālmīki once stood (though the Gaṅgā is mentioned, neither of the places is named in *Rāmāyaṇa*, cf. 7.44.15ff.). Earlier explorations at the site had revealed copper harpoons and

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[50] Source: *IAR 1978-79*: 61-62; Lal and Dikshit 1982.

spear-heads typical of the Copper Hoard Culture; later sherds of the PGW were also discovered (cf. *IAR 1953-54*: 38; also cf. *IAR 1961-62*: 57; *IAR 1962-63*: 39). Though the suggestion for excavating the site was made, it was not carried out (cf. *IAR 1953-54*: 42).[51]

On the ancient mound called *gaṛhī*, otherwise thickly populated, five trenches were laid. Only one of these digs, PRR-3, clearly indicated the sequence of cultures. The OCP, black-slipped, and black and red ware were found in a mixed context at the lowest levels.[52] The latter two continued in the next period along with the red ware. The third and fourth periods were marked by the PGW and NBPW respectively. The Śuṅga–Kuṣāṇa was the fifth period.

Nothing important came forth from the lowest levels. From the second period, apart from the pottery mentioned above, terracotta beads and discs, bone points, copper and stone artefacts were collected. Post-holes of various sizes with plaster and reed-impression, suggesting some kind of structure, were also noticed. From the PGW levels, *ghaṭa*-shaped beads, terracotta discs and bone points were recovered. As in Ayodhyā, the NBPW phase here could be divided into two sub-phases: the earlier was devoid of miniature bowls, carinated *hāṇḍī*s and Ahichchhatra type 10A; they appear in the second sub-phase. With these remains were found, of terracotta, female figurines, wheels, skin-rubbers, beads, discs, bangles and gamesmen, a ring-well, bone points and antimony rods of copper. That burnt bricks were manufactured at the site is clear from the remains of brick kilns. The last phase,

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[51] The black and red ware of UP and those from Āhaṛ (Rajasthan) have no common characteristics; in the former region they "consist mostly of dishes and bowls which are devoid of any painting" whereas "Āhaṛ has yielded its own characteristic shapes and painted designs" (*IAR 1979-80*: 31; Lal and Dikshit 1982: 31).

[52] A later publication includes burnished grey ware to this repertoire; this ware was absent in the following stage (cf. Lal and Dikshit 1982: 26-27).

the Śuṅga–Kuṣāṇa, is represented by "flimsy deposit".

CITRAKŪṬA[53]

Though the site of Citrakūṭa (Dist. Bāndā) is mentioned in the epic several times (cf. 1.1.26c and 28a; 2.48.26c, 34d, etc.), it was not subjected to excavation. During the exploration of the site, sherds of the black-slipped ware, NBPW, and associated red wares were picked up. A few red ware sherds of a later period were also met with.

BHARDVĀJA ĀŚRAMA (DIST. ALLAHABAD)[54]

The place is mentioned in *Rāmāyaṇa* (2.48.7 onwards; 2.84.1, etc.). During the exploration and excavation at the site, it was observed that the place must have once stood right on the banks of the Gaṅgā. Here trench ALB-3 revealed the existence of the black-slipped ware, grey ware, NBPW and associated red ware, along with a "few charcoal bits and lumps of clay with reed-impressions". The latter evidence suggests "a casual habitation" in this area. In the words of the excavator,

> the deposit yielding stray sherds of the NBP ware was that of sandy loam and not of regular house-floors. The presence of reed-impressed clay-plaster clearly indicates that on the sandy bank of the river there stood a few huts — a scenario which fits well into that of a hermitage. Whose hermitage it was, unfortunately archaeology will have to remain dumb since at that point of time writing was not used in the Gaṅgā valley.
>
> — 1993: 7

The site was again populated during the Gupta period. Lal wants to connect the reoccupation of the site with the rising popularity of the epic during and after the Gupta period, which is clearly visible in the numerous *Rāmāyaṇa* episodes carved on the temple panels in India and, a little later, abroad (cf. Kala 1988: 17-48; Vatsyayan 2004, for a survey). From this period were recovered an inscribed

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[53] Source: *IAR 1980-81*: 70.

[54] *IAR 1978-79*: 56; *IAR 1982-83*: 90.

seal, a few sealings, terracotta figurines and pottery. An inscribed sealing with the figures of Gaṅgā and Yamunā was also found.

ŪÑCHĀGĀON MAJHGĀVAN

There are some more sites which are associated with the epic but were not taken into account by Lal. For instance, Ūñchāgāon Majhgāvan, which is said to be related to the Mahāgrāma of *Rāmāyaṇa* and Majhgāvan (?) of the Pāli texts.[55] Kuṣāṇa and Gupta period potteries were collected from here but no pottery of the older periods was noticed. A seal with the figure of a horse and the legend *aśvaghoṣasya* was also found from here (*IAR 1991-92*: 105).[56] From earlier explorations at the site the grey ware, red ware and medieval glazed ware (*IAR 1963-64*: 54) and some medieval sculptures were reported (*IAR 1970-71*: 80).

SAṄKISĀ

Saṅkisā is another site (Dist. Farrukhābād, UP). It is identified with Saṅkāsyā, the capital of Kuśadhvaja, the younger brother of King Janaka, the father of Sītā[57] (cf. *Rāmāyaṇa* 1.69.3c, 1.70.16b, 19c), and Saṅkāssa of the Buddhist literature (cf. Malalasekera 1937, s.v.). It was here that the Buddha descended from the Trāyastriṁśa Heaven in order to preach the gods and his mother. No wonder that Aśoka chose to erect a column with

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[55] Mahāgrāma is mentioned but once in *Rāmāyaṇa* (4.39.21c), not as the name of a place but of a "country" (cf. Mankad 1965: xliv). In any case, the *sarga*s in question (39-42) are borrowed from the Purāṇas at a later date (cf. Mankad 1965: xxxvff.), and therefore the mention is not of much consequence. In Pāli literature, I have been unable to locate the word "Majhgāvan". The word *mahāgrāma* occurs in the Sūtra literature, where, according to Wagle, it is equivalent of *nigama* meaning "a market town", "a town", "a township", or "a district" (1966/1995: 21).

[56] Would it have any connection with the famous Buddhist poet Aśvaghoṣa?

[57] *Rāmāyaṇa* (1.69.3b) locates it on the banks of the River Ikṣumatī, which is the modern Īkhan aka Kālī Nadī.

elephant capital here[58] and possibly built a *stūpa* too, remnants of which are still visible. B.B. Lal had explored the site earlier and reported the PGW and NBPW (cf. *IAR 1955-56*: 71). In later explorations, apart from potteries mentioned above, terracottas, coins, sculptures, etc. were noticed. The site was later excavated under the direction of B.R. Mani. During the excavation, five cultural phases were brought to light: the PGW, NBPW, Śuṅga, Kuṣāṇa and Gupta. From the PGW layers, along with associated potteries, broken female figurine, rattles, discs, etc. of terracotta, bone points and arrowheads, iron chisel and a stone bead were recovered. A mud floor, 8 cm thick, with circular post-holes was found from the early NBPW period. A burnt brick wall belonging possibly to the Kuṣāṇa period was also unearthed. It is not clear if burnt bricks were used during the NBPW period, but it was certainly in use in the subsequent periods as some structures made of it belonging to the Śuṅga, Kuṣāṇa and Gupta periods testify.

The recovery of no less than 343 antiquities from the first season and 325 from the second season clearly shows that it was a prosperous place (*IAR 1995-96*: 89-97; *IAR 1996-97*: 139-42).

BASĀṚH

The site of Basāṛh corresponds to the ancient twin towns of Viśālā (Vaiśāli) and Mithilā. The latter was the capital of Janaka, Sītā's father (cf. *Rāmāyaṇa* 1.12.18c; 1.47.10; 1.64.29b, etc.). The site was subjected to excavation many times (for a summary, see, Sinha 1989). The earliest habitation at the site in the *gaṛh* area is represented by the black and red ware with the NBPW and, therefore, datable to *c.* 600–500 BCE. A few sherds of the so-called degenerate grey ware with paintings, comparable to those from Ayodhyā, were also found.

Discussion

These, in short, are the results of the various explorations and

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58 This capital is similar to the one discovered from Sodhanga (Dist. Ujjain, MP) by Wakankar (cf. *IAR 1966-67*: 82).

excavations carried out at the sites associated with *Rāmāyaṇa*. As can be seen from the précis of the excavation reports, no positive evidence could be found that would confirm the historicity of the epic. This is due no doubt to an inherent epistemological impasse. It has been insisted that only an indubitable proof, like an inscription, can prove the veracity of the story. The script being non-existence before the beginning of the fourth century (cf. Coningham et al. 1996), this proof can never be furnished. Circumstantial evidence, at times, can help us. The evidence tells us that all the *Rāmāyaṇa* sites were contemporary. Śṛṅgaverapura, Pariār, and possibly Mithila, pre-existed before the other sites, true, but they were active during the NBPW period, too. The site known and identified as Bhardvāja Āśrama was inhabited only during the NBPW period (that it was reoccupied during the Gupta era is a different thing), and it was not a regular habitation site. If the identification of the sites is correct, it can be argued that the kernel of the *Rāmāyaṇa* story cannot be earlier than the seventh or eighth century BCE.[59] The same date is suggested on the ground of textual data (cf. fn. 37).[60]

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[59] The dating of Ayodhyā proposed on the ground of radiocarbon dates "c. 1300 B.C. (1250 ± 130 B.C.)", cf. Dikshit 2003: 117) is too anomalous to be seriously considered, not to mention the "material evidence" — which by the way includes "a round signet with legend in Aśokan Brāhmī" — that speaks for a much, much later date.

[60] If we accept the connection between Hāstinapura and the *Mahābhārata* story and between Ayodhyā and the *Rāmāyaṇa* story, it would mean that *Rāmāyaṇa* is later than *Mahābhārata*. It has been argued that according to the doctrine of *daśāvatāra* Rāma belong to the Tretā and Kṛṣṇa to the Dvāpara-yuga; the former comes before and the latter later. Therefore, *Rāmāyaṇa* cannot be later than *Mahābhārata* (cf. B.P. Sinha quoted in Dikshit 2003: 117). The contention is weak. First, both the theory of the four *yuga*s and the belief of the *daśāvatāra* are later than the epics. As González-Reimann's work on *Mahābhārata* competently shows, the *yuga* theory was superimposed, albeit craftily and intelligently, on the epic at a later date (cf. 2002). Second, *Rāmāyaṇa* itself places Rāma in the Dvāpara-yuga (cf. 7.65.19c; 7.65.20a; 7.65.23b).

The plausible dating of the story, however, does not automatically validate the existence of persons and events mentioned in it. The text of the epic was constantly revised and inflated until very late; naturally, it covers many centuries and therefore many cultural phases. A successful correlation of the literary and archaeological data could have offered a possible date for the various strata of the text and that would have been a decent contribution to textual and archaeological study of the epic. It is a pity that in the project on the archaeology of *Rāmāyaṇa* no trouble was taken to do this. From the reports, the excavators appear to have been concerned only with the cultural sequence as revealed through pottery types. The reports usually, at times even exclusively, are concerned with the description of pottery even though the shapes, types, etc. of the cultures are fairly known; the other antiquities are seldom described. B.R. Mani, for instance, in his report of the first season of the excavation at Saṅkisā devotes nearly five pages (with drawings) out of eight to pottery description whereas three hundred and forty three antiquities — terracotta discs with a variety of decoration, animal and human figurines, toy-cart wheels, beads, gamesmen, ivory objects, a copper coin, etc. — are cursorily mentioned in just one paragraph of twelve lines (cf. *IAR 1995–96*: 89–97). In the second season, too, many human and animal figurines, terracotta beads and pendants, bangles, discs, wheels, numerous miscellaneous objects, stone beads, five copper coins, pieces of glass bangles and beads, numerous metal objects, shell bangles, a lithic human head, etc. were unearthed, but none is described (cf. *IAR 1996–97*: 142). The same applies to excavated material from the other *Rāmāyaṇa* sites — the only (partial) exception being that of Śṛṅgaverapura, the first volume of the proposed full report was published later.

If we wish to illuminate literary traditions with the help of archaeology, it is quite obvious that merely the antiquity of the sites, or the cultural sequence of the sites, or the description of the pottery types, etc. will not help. Unfortunately, the excavators

stopped where the actual work should have begun. We can only imagine how a Vasudev Sharan Agrawala or a Motichandra would have work on this enviably rich archaeological material. H.D. Sankalia's insightful correlation of archaeological findings with the textual data (for example, that of the signet ring, the Roman amphorae, the description of the cities, the *pādukā*s of Rāma, etc.) could have served as a model for the excavator.[61] An example may help understand this. A gigantic tank was unearthed from Śṛṅgaverapura. Instead of searching any clue to it from *Rāmāyaṇa*, Lal sought to attribute it to Dhanadeva of the Ayodhyā inscription (Lal 1993: 47-48). The question is not that his attribution is wrong (or right for that matter); the issue is, for an investigator of a project on the epic, it would have been worthwhile to look into the text to find any possible reference to the tank in it. It was Sankalia who boldly put forward the view that the tank could be related with the terrible drought mentioned in *Rāmāyaṇa* (1.8.12ff., cf. Sankalia 1984). I do not suggest that Sankalia's guess is correct — there were many more famines in ancient India (cf. Biswas 2000; Ganguli 1934 for textual references), and the author of the verse in question might be referring to any of them, we are not sure to which — but the approach certainly is. The deplorable lack of full reports and of good reproductions of the antiquities or their detailed description will surely hinder the progress of archaeological understanding of the epic in the coming years. Viewed from this perspective, despite giving us a possible terminus post quem for the epic, the *Rāmāyaṇa* Project disappoints us.

Lal had clearly announced his objective behind taking up the *Rāmāyaṇa* Project that it was 'to find out its [*scil*. Ayodhyā's] antiquity and to ascertain if and what light it can (or even cannot) throw on the historicity of the *Rāmāyaṇa*?' (1981: 49) Our answer to this question is plain: though our inferences are

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[61] No wonder that Nilmadhav Sen, a life-long student of *Rāmāyaṇa*, speaks very highly of Sankalia's contributions to the *Rāmāyaṇa* studies (Sen 1989; also cf. Lariviere 1986).

only provisional and stand to improvement and correction, our current knowledge tells us that the site is not any more ancient than the seventh-eighth century BCE and that it does not throw any light on the historicity or otherwise of *Rāmāyaṇa*.

One need not be disheartened with the conclusions reached in this note, for, as H.D. Sankalia correctly observed, 'the truths enshrined in these works — Śruti, Smṛti, Purāṇas — are eternal, and cannot be shaken by such studies' (Sankalia, 1976: 4).

All those who wish to interpret the archaeological record otherwise, must remember Rāmā's these words: *ahaṁ satyamicchāmi nānṛtam* (2.31.32).

Acknowledgement

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